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A NATURALIST'S YEAR.

By GRANT ALLEN.

BLACKBERRIES ARE RIPE.

IN all the hedgerows and on all the commons, now, the village children are eagerly filling crocks and baskets with the one great fruit of the year to them, poor little souls, the common wild blackberries. There is no bush more familiar in overgrown spots in England than the bramble, and yet there are few others the story of whose evolution is more interesting than that of this universally distributed British shrub. We can trace the entire history of the blackberry through its various gradations almost without transgressing beyond the limits of our own little native flora. The bramble kind are a special offshoot of the rose family, distinguished chiefly by their peculiar granulated berries, each of which consists of several tiny one-seeded, succulent fruitlets, united round the common receptacle into a single compound fruit. The origin of the race from the original central rosaceous stock, represented by the cinquefoil and other potentillas, is so clear and obvious that it well repays a few minutes' careful attention.

In their simplest existing forms, the rosaceous plants are low perennial herbs, with small yellow flowers, and tiny dry nut-like seeds, crowded together on a large flat or conical receptacle. This is the type familiar to all of us in the English cinquefoils, silverweeds, and tormentils; and from some such form, the whole of the widely varying rose family has slowly diverged in one direction or another. But it is not often that we can trace the steps of the divergence among surviving plants so clearly as in the case of the British blackberries. The lowest existing members of the blackberry genus are small creeping herbs, of simple habit, hardly differing at all from the potentillas except in the peculiar character of their granulated fruit. Of these very primitive kinds, the cloudberry of Northern Europe may be taken as an excellent example. It is a little unobtrusive plant, growing abundantly in the turfy bogs and tundras of Scandinavia, Russia, and Siberia, and still found in quantities in Scotland, though rare in Northern England and the Welsh hills. Like many other early forms, in fact, the cloudberry has now been stranded in Arctic or Alpine situations, while both it and its relatives have been driven entirely from more favoured southern climates by its own more advanced cousins, the raspberry, the blackberry, and the dewberry. The stem bears no prickles, and the leaves are simple and rounded in outline, or at most slightly lobed, instead of being deeply divided into separate leaflets, as in all the more advanced bramble types. Its flowers are large, as is often the case with Arctic blossoms, so as to attract the eyes of the rare northern butterflies or moths; and in this respect the cloudberry is just abreast of some higher potentillas, like the barren strawberry and the true strawberry, which have also progressed from yellow to white petals; while it is ahead of some other rather more developed members of its own genus, which, though in most respects superior to it, have not yet got beyond the primitive stage of yellow flowers. But it is the possession of the peculiar berry which at once marks off the cloudberry from the potentilla group, and points at its true place as an early embodiment of the blackberry type. This berry has been formed from the numerous hard dry carpels or nutlets of the potentillas through the selective agency of the northern birds, in whose scanty diet they form as important a part as they do in the usual stores of Arctic travellers. The outer coat of each little nut has grown soft and succulent, and has at the same time acquired a bright orange-red colour, to attract the attention of the friendly birds by whose aid its seeds are dispersed. The device is exactly analogous to that adopted unconsciously by the strawberry, only that in the strawberry it is the receptacle that becomes sweet and brightly-coloured, while the individual "seeds" or carpels (really small nuts) remain hard and unobtrusive: whereas in the cloudberry the receptacle is inconspicuous and dry, while the outer coat of the carpels has become succulent and ruddy. The mulberry (a tree belonging to a widely different family—that of the figs and nettles) has a granulated fruit even closer in some respects to that of the bramble: but each granule of the mulberry is produced from the carpels of a separate flower, while in the blackberry the whole set of carpels belong to a single blossom.

There are other little herbaceous bramble kinds besides the cloudberry, such as the pretty little *Rubus arcticus*, a very northern form, whose flowers have become pink, while its leaves are divided into three leaflets, like those of the strawberry; but these are of less importance in the genealogical order than our own British stone-bramble, a mountain plant of central and northern Europe, found pretty frequently in Scotland, in Yorkshire, and in the Welsh hills. In this ugly but interesting little plant, the buried rootstock sends up short greenish stems, very slightly armed with the first beginnings of prickles in the shape of small swollen and pointed projections. Sometimes, indeed, these rudimentary defences are altogether wanting; at other times they assume the form of fairly developed sharp spines. This is just what one would expect from the nature of the spots which the stone-bramble inhabits: a low, spreading herb, like the cloudberry, growing in broad northern bogs and tundras, has little need of protection against browsing herbivores; but comparatively tall and juicy stems, like those of the stone-brambles, growing in open woods or on broken mountain sides in the great backbone ridges of Europe and Asia, would soon get eaten down entirely by chamois, sheep, or other wild and domesticated ruminants, unless they were well protected by thorns or prickles from their herbivorous foes. How quickly the weeding action of the animals can produce the survival of protected specimens only is well seen on all the suburban

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commons of Kent and Surrey, where the prickly variety of common rest-harrow alone can thrive; the more ordinary armed variety is all eaten down wherever it appears by the ubiquitous London donkey. The leaves of the stone-bramble are divided into three leaflets, after a fashion which runs more or less throughout the whole genus; and the flowers are in a sort of intermediate stage between dirty greenish-yellow and dingy yellowish-white. The red berries do not differ much, except in the fewness of the carpels, from those of the raspberry.

In the higher brambles—the raspberry, blackberry, and dewberry—we get the same type still further developed into a straggling woody bush. This growth in woodiness depends, of course, merely on the thickening and lengthening of certain cells and cell-walls in the stem, and it is everywhere readily produced by natural selection, wherever the circumstances are favourable to its evolution. Yet the difference between the stone bramble and the raspberry in this respect is far less than one might at first imagine; both have perennial creeping root-stocks as reserves of material; but that of the stone-bramble only sends up green annual shoots, herbaceous in character; while that of the raspberry sends up rather stouter and woodier biennial stems, which seldom outlive the second year. In the blackberry, they sometimes continue for three or four years together. Such gradual intermediate stages are almost universal in nature. At the same time that the higher brambles have acquired their woody and creeping habits, they have also acquired sharp prickles, rather weak in the raspberry, stout and usually hooked in the blackberry. These prickles not only serve to defend the plants against the cattle that browse (or the deer that once browsed) among the thickets where they love to grow, but also aid them in clambering over the bushes, hedges, or heaps of stones over whose top they usually straggle. The raspberry flowers are white; but the blackberry often shows a tinge of pink in its much larger petals which reminds us of such bigger and more developed rose-flowers as apple blossom, cherry blossom, and the true roses. Side by side with these changes, the leaves, now growing out into the open by the aid of the woody stems, can afford to expand more freely and widely; and so, instead of the simple lobed leaf of the cloudberry, or the trefoil leaf of the stone-bramble, these higher types have usually leaves of five leaflets, though the gradation from the three-leaved to the five-leaved form can almost always be observed upon the same plant. In the wild raspberry, as everybody knows, the fruit is still red, like that of most other brambles, but the blackberry seems to suit our native birds better, with its dark purplish-blue tinge, than any of these brilliant northern kinds. Indeed, it is noticeable that very dark-blue and black, which are extremely rare colours in flowers, are extremely common in wild fruits—for example in sloes, privet, whortleberries, wild madder, elderberries, dogwood, and wayfaring-trees. This seems to suggest a difference of taste in colour between birds and insects. The true blackberry has no bloom on the fruit, but in the variety known as the dewberry, and commonly accounted a species, the berry is covered with a delicate blue-black mealiness which makes it a very pretty object indeed. Blackberry brambles in fact, show an immense tendency towards variation—so much so, that while Mr. Bentham makes two British species, and Sir W. Hooker six, Professor Babington actually distinguishes as many as forty. As yet, however, it does not seem to me that any special selective action has been exerted upon these numerous varying forms, and, therefore, from the evolutionary point of view, they must be regarded as mere accidental sports, not as true botanical species.

THE CHEMISTRY OF COOKERY.

By W. Mattieu Williams.

XIX.

REFERRING to No. 17 of this series, August 1, a correspondent who has just returned from Norway, where he followed the route of my last trip there, reminds me of the marvellous congregation of sea-birds that assembles on some of the headlands of the Arctic Ocean, and suggests that egg-oil might be obtained in large quantities there. He quotes from the work of P. L. Simmonds on "Waste Products" the following:—"In the Exhibition of 1862 the Russian Commission showed egg-oil in large quantities and of various qualities, the best so fine as to far excel olive oil for cooking purposes;" but it was not sufficiently cheap for general use.

Among the places indicated by Mr. Grimwood Taylor, the most remarkable is Sverholt Klubben, a grand headland between the North Cape and Nord Kyn, rising precipitously from the sea to a height of above 1,000 feet. The face of the rock weathers perpendicularly, forming a number of ledges about two or three feet above each other, and extending laterally for more than a mile. On the two occasions when I passed it, the whole of this amphitheatre was occupied by a species of gull, the "kittiwake," perched on the ledges, their white breasts showing like the shirt-fronts of an audience of a million or two of male pigmies in evening dress. On blowing the steam-whistle, the rock appeared to advance, and presently the sky was darkened by a living cloud, and every other sound was extinguished by a roar of wings and the harsh wailing screams of a number of birds that I dare not estimate. The celebrated bird colony on the Bass Rock is but a covey compared with this.

The inhabitants of the little human settlement in the Bay of Sverholt derive much of their subsistence from the eggs of these birds; but whether they could gather a few millions for oil-making without repeating the story of the goose and the golden eggs, is questionable. The eider-ducks that inhabit some of the low mossy islands thereabouts, are guarded by strict legislative regulations during their incubation period, lest they should emigrate, and the down-harvest be sacrificed.

I now come to the subject of *stewing*, more especially the stewing of flesh food. Some of my readers may think that I ought to have treated this in connection with the boiling of meat, as boiling and stewing are commonly regarded as mere modifications of the same process. According to my mode of regarding the subject, *i.e.*, with reference to the object to be attained, these are opposite processes.

The object in the so-called "boiling" of, say, a leg of mutton is to raise the temperature of the meat throughout just up to the cooking temperature (see Nos. 3 and 4) in such a manner that it shall as nearly as possible retain all its juices; the hot water merely operating as a vehicle or medium for conveying the heat.

In stewing nearly all this is reversed. The juices are to be extracted more or less completely, and the water is required to act as a solvent as well as a heat-conveyor. Instead of the meat itself surrounding and enveloping the juices as it should when boiled, roasted, grilled, or fried, we demand in a stew that the juices shall surround or envelope the meat. In some cases the separation of the juices is the sole object, as in the preparation of certain soups and gravies, of which "beef-tea" may be taken as a typical example. *Extractum Carnis*, or "Liebig's

Extract of Meat" is beef-tea (or mutton-tea) concentrated by evaporation.

The juices of lean meat may be extracted very completely without cooking the meat at all, merely by mincing it and then placing it in cold water. *Maceration* is the proper name for this treatment. The philosophy of this is interesting, and so little understood in the kitchen that I must explain its rudiments.

If two liquids capable of mixing together, but of different densities, be placed in the same vessel, the denser at the bottom, they will mix together in defiance of gravitation, the heavy liquid rising and spreading itself throughout the lighter, and the lighter descending and diffusing itself through the heavier.

Thus, concentrated sulphuric acid (oil of vitriol) which has nearly double the density of water, may be placed under water by pouring water in a tall glass jar, and then carefully pouring the acid down a funnel with a long tube, the bottom end of which touches the bottom of the jar. At first the heavy liquid pushes up the lighter, and its upper surface may be distinctly seen with that of the lighter resting upon it. This is better shown if the water be coloured by a blue tincture of litmus, which is reddened by the acid. A red stratum indicates the boundaries of the two liquids. Gradually the reddening proceeds upwards and downwards, the whole of the water changes from blue to red, and the acid becomes tinged.

Graham worked for many years upon the determination of the laws of this diffusion and the rates at which different liquids diffused into each other. His method was to fill small jars of uniform size and shape (about 4 oz. capacity) with the saline or other dense solution, place upon the ground mouth of the jar a plate-glass cover, then immerse it, when filled, in a cylindrical glass vessel containing about 20 oz. of distilled water. The cover being very carefully removed, diffusion was allowed to proceed for a given time, and then by analysis the amount of transfer into the distilled water was determined.

I must resist the temptation to expound the very interesting results of these researches, merely stating that they prove this diffusion to be no mere accidental mixing, but an action that proceeds with a regularity reducible to simple mathematical laws. One curious fact I must mention—viz., that on comparing the solutions of a number of different salts, those which crystallise in the same forms have similar rates of diffusion. The law that bears the most directly upon cookery is that "the quantity of any substance diffused from a solution of uniform strength increases as the temperature rises." The application of this will be seen presently.

It may be supposed that if the jar used in Graham's diffusion experiments were tied over with a mechanically air-tight and water-tight membrane, that brine or other saline solution thus confined in the jar could not diffuse itself into the pure water above and around it; people who are satisfied with anything that "stands to reason" would be quite sure that a bladder which resists the passage of water even when the water is pressed up to the bursting-point, cannot be permeable to a most gentle and spontaneous flow of the same water. The true philosopher, however, never trusts to any reasoning, not even mathematical demonstration, until its conclusions are verified by observations and experiment. In this case all rational preconceptions or mathematical calculations based upon the amount of attractive force exerted between the particles of the different liquids are outraged by the facts.

If a stout, well-tied bladder that would burst rather than allow a drop of water to be squeezed mechanically through it be partially filled with a solution of common washing soda, and then immersed in distilled water, the soda will make its way out of the bladder by passing through its walls, and the pure water will go in at the same time; for if, after some time is allowed, the outer water be tested by dipping into it a strip of red litmus paper, it will be turned blue, showing the presence of the alkali therein, and if the contents of the bladder be weighed or measured, they will be found to have increased by the inflow of fresh water. This inflow is called *endosmosis*, and the outflow of the solution is called *exosmosis*. If an india-rubber bottle be filled with water and immersed in alcohol or ether, the endosmosis of the spirit will be so powerfully exerted as to distend the bottle considerably. If the bottle be filled with alcohol or ether and surrounded by water it will nearly empty itself.

The force exerted by this action is displayed by the rising of the sap from the rootlets of a forest giant to the cells of its topmost leaves. Not only plants, but animals also, are complex osmotic machines. There is scarcely any vital function—if any at all—in which this osmosis does not play an important part. I have no doubt that the mental effort I am at this moment exerting is largely dependent upon the endosmosis and exosmosis that is proceeding through the delicate membranes of some of the many miles of blood-vessels that ramify throughout the grey matter of my brain. But I must wander no further beyond the kitchen, having already said enough to indicate that exosmosis is fundamental to the philosophy of beef-tea extraction, and reserve further particulars for my next paper.

Postscript.—I feel bound to step aside from the proper subject of these papers to make public acknowledgment of an act of honourable generosity, especially as many hard things have been said concerning American plagiarism of the work of British authors. As everybody knows, we have no legal rights in America, and any publisher there may appropriate as much of our work as he chooses. American *legislators* are responsible for this. Nevertheless, I received, a short time since, a letter from Mr. E. L. Youmans, of New York, enclosing a cheque for £20, as an honorarium in consideration of the fact that these papers are being reprinted in the *Popular Science Magazine*. Shortly before this, a similar remittance was sent from another publisher (Messrs. Funk & Wagnalls), who have reprinted "Science in Short Chapters." These facts indicate that some American publishers have larger organs of conscientiousness than the present majority of American legislators.

I am told that another American publisher has issued another reprint of "Chemistry of Cookery" without making any remittance; but, as Mr. Proctor would say, "this is a detail."

THE ANTIQUITY OF MAN.*

By W. PENGELLY.

PREVIOUS to 1858 all geological evidence respecting the antiquity of man was received with apathy and indifference. Early in the present century the Rev. J. M'Enery discovered flint implements beneath a thick continuous sheet of stalagmite in Kent's Cavern, Torquay, but when he submitted these specimens to Dr. Buckland, then the leading geologist of the day, the latter refused to regard them as evidences of the antiquity of man, but was of opinion they belonged to the ancient Britons, who "had scooped out ovens in the stalagmite," thus

* Abstract of an address delivered at the British Association.

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accounting for their presence in the diluvium. This opinion Dr. Buckland held notwithstanding Mr. M'Enery's statement that in no instance had he discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam. In justification of Dr. Buckland's conduct, it should be mentioned that he himself had explored Kent's Cavern previously to Mr. M'Enery's researches therein, and, while doing so, had discovered a flint implement. This, however, he found under such circumstances as did not conflict with his published opinion on the low antiquity of man.

But it is doubtful whether even Dr. Buckland's faith in his early convictions remained unshaken to the end, for when pressed by an intimate friend, a Professor at Oxford, to prepare a new edition of his "Reliquæ Diluvianæ" and his "Bridgewater Treatise," he excused himself on the ground that the work would not be editing "but rewriting."

In 1840 Mr. Godwin Austen, F.G.S., read a paper before the Geological Society of London on the Bone Caves of Devonshire. Speaking of Kent's Cavern, Mr. Austin said that human remains and works of art, such as arrow-heads and knives of flint, occurred in all parts of the cave, and throughout the entire thickness of the clay, and no distinction founded on condition, distribution, or relative position, could be observed, whereby the human could be separated from the other *reliquæ*. He also expressed the opinion that the bones and works of art must have been introduced into the cave before the flooring of stalagmite had been formed. These statements, however, attracted little or no attention at the time.

In 1846 the Torquay Natural History Society appointed a committee, consisting of Dr. Battersby, Mr. Vivian, and Mr. Pengelly, to make a few diggings in Kent's Cavern for the purpose of obtaining specimens for their museum. In the report of their investigations this committee stated that they had established the important point that relics of human art were found beneath the unbroken floor of stalagmite. After taking every precaution by sweeping the surface and examining most minutely whether there were any traces of the floor having been previously disturbed, they broke through the solid stalagmite in three different parts of the cavern, and in each instance found flint knives. In the spot where the most highly finished specimen was found, the passage was so low that it was extremely difficult with quarrymen's tools and good workmen to break through the crust, so that the supposition that it had been previously disturbed was impossible. But such was the incredulity with which the inferences deducible from these facts were received that when the report was printed in the "Quarterly Journal" of the Geological Society, it was formally announced that the authors alone were responsible for the facts and opinions contained in their respective papers.

This state of incredulity and apathy lasted until 1858, when some workmen engaged in a limestone quarry on Windmill-hill, near the fishing town of Brixham, in South Devon, unexpectedly broke a hole through what proved to be the roof of an unknown and unsuspected cavern. A committee of exploration was immediately appointed and placed under the superintendence of Professor Prestwich and Mr. Pengelly. The facts which mainly contributed to the decision to have this cave systematically explored were that it was a virgin cave which had been hermetically sealed during an incalculably long period, the last previous event in its history being the introduction of a reindeer antler found attached to the upper surface of the stalagmite floor, and therefore it was free from the objection sometimes urged against Kent's Cavern—namely, that, having been known from time immemorial, and up to 1825 always open to all comers, it had perhaps been ransacked again and again: secondly, it was believed and proved to be a comparatively small cavern, so that its complete exploration was not likely to require a large expenditure of time or money.

At the end of a period of twelve months, during which time the cave had been subjected to a thorough and searching investigation, the committee were able to report that eight flint tools had been found in various parts of the cavern, all of them inosculating with bones of mammalia, at depths varying from nine to forty-two inches in the cave earth, on which lay a sheet of stalagmite from three to eight inches thick, and having within it and on it, relics of lion, hyæna, bear, mammoth, rhinoceros, and reindeer.

The results of these explorations at Brixham had an enormous influence in impressing the scientific world generally with the value and importance of the geological evidence of man's antiquity.

Among the first fruits of the awakening was a paper by Professor Prestwich, read to the Royal Society, May 26, 1859, on the occurrence of flint implements associated with the remains of animal of extinct species in beds of a late geological period in France at Ameins and Abbéville, and in England at Hoxne. In this paper Professor Prestwich distinctly stated that it was the discoveries which he had witnessed at Brixham which first fully impressed him with the validity of the doubts thrown upon the previously prevailing opinions with respect to such remains in caves. Sir Charles Lyell, too, in his address to the Geological Section of the British Association at Aberdeen, September, 1859, stated that the facts brought to light in connection with the explorations in the Brixham Cave had prepared geologists to admit that scepticism in regard to the cave evidence in favour of the antiquity of man had been pushed to an extreme.

But probably the greatest proof of the change of opinion which began to take place in the scientific world in regard to this subject was that no less than three editions of Sir Charles Lyell's bulky work on the "Antiquity of Man," which first appeared in February, 1863, were published in the course of ten months.

The result of the researches at Brixham quickened a desire to re-examine the Kent's Cavern evidence, and, accordingly, in 1864, a committee was formed, and received a grant of money from the British Association for that purpose. The investigation was begun on March 28, 1865. The committee was annually reappointed, and the work continued without intermission to June 19, 1880. The total money grants amounted to £1,900, together with £63 received from various private sources.

In M'Enery's work was a diagram of three remarkable canine teeth, belonging to a group of carnivoræ to which in 1846 Professor Owen gave the name of *Machairodus latidens*. A considerable amount of scepticism existed for many years as to whether these specimens were really found in Kent's Cavern, it being contended that, from its zoological affinities, *Machairodus latidens* must have belonged to an earlier fauna than that represented by the ordinary cave mammals. It was therefore naturally hoped that the re-exploration of the cavern would set this question at rest.

However, it was not until after the lapse of seven years and four months that the President, while engaged in washing a "find," discovered a well-marked incisor of *Machairodus latidens* with a left ramus of lower jaw of

bear, in which was one molar tooth. This at once established M'Enery's accuracy, left no doubt that *Machairodus latidens* was a member of the cave earth fauna—whatever the zoological affinities might say to the contrary—and proved that man and *Machairodus* were contemporaries in Devonshire.

When their exploration began, and for some time afterwards, the committee had no reason to suspect that the cavern contained anything older than cave earth. At the end of the first five months, however, facts pointing apparently to earlier deposits began to present themselves, and when, after the expiration of three years, a vertical section was cut, there was shown in clear, undisturbed succession, not only the cave earth with the granular stalagmite lying on it, but under, and apparently supporting the cave earth, another thicker and continuous sheet of stalagmite called crystalline, and below this, again, an older detrital accumulation known as the Breccia, made up of materials utterly unlike those of cave earth. The Breccia was just as rich as the cave earth in osseous remains, but the lists of species represented by the two deposits were very different. The remains of the hyæna prevailed numerically very far above those of any other mammal in the cave earth, his presence being attested by his teeth-marks on a vast number of bones, by lower jaws—including those of his own kith and kin—of which he had eaten off the lower borders as well as the condyles, by long bones broken obliquely just as hyænas of the present day break them, and by surprising quantities of his coprolites. In the Breccia, however, there was not a single indication of his presence, the crowd of bones and teeth belonging almost entirely to bears. No trace of man was found in the Breccia until March, 1870, when a flint flake was met with in the third-foot level, which was believed to be not only a tool, but to bear evidence of having been used as such. Two massive flint implements were discovered in the same deposit in May, 1872.

At various times other tools were found, until at the close of the exploration the Breccia had yielded upwards seventy implements of flint and chert. All the stone tools, both of cave earth and Breccia, were Palæolithic, and were found inosculating with the remains of extinct mammals, but a cursory inspection showed them to belong to two distinct categories. Those found in the Breccia, the more ancient series, were formed by chipping a flint nodule or pebble into a tool, while those from the cave earth were formed by first detaching a suitable flake from the nodule, or pebble, and then trimming the flake—not the nodule—into a tool.

The fact, however, most significant of time and physical change was the presence of the hyæna in the cave earth or less ancient, but not in the Breccia or more ancient of the two deposits. This fact rendered it almost impossible to avoid the conclusion that the hyæna was not an occupant of Britain during the earlier period. The acceptance of such a belief, however, would necessitate the adoption of the view that man was resident in Britain long before the hyæna, and also that it was possible for the hyæna to reach Britain some time between the deposition of the Breccia and the deposition of the cave earth. In other words, that Britain was part of the continent during this interval.

In support of this argument, it is to be remembered that Sir Charles Lyell recognised the following geographical changes within the British area between the newer pliocene and historical times—firstly, a pre-glacial continental period, towards the close of which the Forest of Cromer flourished and the climate was somewhat milder than at present; secondly, a period of submergence, when the land north of the Thames and Bristol Channel and that of Ireland was reduced to an archipelago. This was part of the glacial age, when icebergs floated in our waters. Thirdly, a second continental period, when there were glaciers in the higher mountains of Scotland and Wales. Fourthly, the breaking up of the land through submergence and a gradual change of temperature, resulting in the present geographical and climatal conditions.

The fact that neither in the Kent's Cavern Breccia, nor in the Forest of Cromer were any remains of hyæna found, and that the list of mammalian remains found in the one does not clash with those found in the other, renders legitimate the inferences that the hyæna did not reach Britain until its last continental period, and that the men who made the Palæolithic nodule tools found in the oldest-known deposit in Kent's Cavern arrived either during the previous great submergence, or, what is more probable, unless they were navigators, during the first continental period. There was little doubt, therefore, but that the earliest Devonians were either of glacial or pre-glacial age. Of course, the discovery of remains of hyæna in the forest bed of Cromer, or any other contemporary deposit, would be fatal to the argument, but it would leave intact all other evidence in support of the doctrine of British glacial or pre-glacial man.

THE SUN'S DISTANCE.*

By Professor R. S. Ball, LL.D.,

Astronomer-Royal for Ireland.

THE problem which is to engage our attention has been justly regarded as one of exceptional interest and importance. It seems not unlikely that in early ages the distance of the sun was one of the very first astronomical problems which ever attracted speculation. In modern times, as the problem has gradually approached solution, the interest attached to it has gradually increased until it has culminated in the last few months by the occurrence of the transit of Venus.

The importance of this problem arises from the fact that the distance of the sun is the base line in terms of which almost every other linear magnitude in astronomy is to be expressed. An accurate measurement of this base will infuse accuracy into all the other astronomical quantities which spring from it. When we have learnt the distance of the sun we can measure the bulk of the sun and his diameter, we can measure the great planet Jupiter or the rings of Saturn, and the scale of the whole solar system becomes known to us. Again, when we attack the loftiest problem in practical astronomy, and seek to stretch a sounding line over the vast abyss which divides our system from the stars, it is the distance of the sun which we must use as our measuring rod. No pains should be spared to give to so fundamental a unit all the precision of which it is capable.

Let us define accurately the magnitude to be measured. The actual distance from the earth to the sun is not constant. In these autumnal months the distance is rapidly decreasing. We are at this moment drawing nearer and nearer to the sun at the rate of a thousand miles an hour. Next Christmas we shall be about a million-and-a-half miles closer to the sun than we are to-night. At the commencement of the new year we shall begin to recede. Next midsummer will find us as far from the sun as possible; then we shall draw in again, arrive next autumn

* A discourse delivered at the second general meeting of the British Association at Southport.

where we are this autumn, and commence anew the cycle of changes I have indicated. Though these changes amount to millions of miles, yet they are at the utmost only a small fraction of the sun's distance. To superficial observation the sun always seems the same size, and hence there can be no great relative changes in its distance.

There is no difficulty is understanding what is meant by the average distance of the sun. To express the idea with precision we may borrow the language of mathematics, and say that the distance from the earth to the sun consists of two parts—a large constant part and a small periodical part. The important problem, and the difficult problem, is the measurement of the large constant part.

The early history of the subject is as easy to sketch as the latter part is difficult. For fourteen centuries the doctrines of Ptolemy were adopted on the distance of the sun as on all other astronomical problems. The method of Ptolemy might have succeeded if the sun's distance could have been measured by thousands of miles instead of by tens of millions. As matters stands, Ptolemy's method was utterly inadequate to cope with the real difficulties of the question. It led him to a conclusion which we now know to have fallen far short of the truth. The real distance of the sun is twenty times as great as that which Ptolemy deduced from his observations. But Ptolemy's result was a great step in advance, notwithstanding the tremendous error by which it was vitiated. It was, at all events, an honest attempt to solve the problem by a direct appeal to nature, and he succeeded so far as to demonstrate the great truth that the sun is larger than the earth.

It is somewhat remarkable that the first reasonable approximation to the sun's distance was obtained by what can only be described as a well-considered guess. The illustrious Huyghens, in the seventeenth century, hazarded a speculation, which seemed plausible at the time, and which we now know to have been reasonably correct. Huyghens compared the diameter of the planet Mars with the sun. He compared the diameter of Venus with the sun. The primitive instruments used were capable of making these measures with some accuracy. Huyghens knew that the earth was also a planet revolving outside the path of Venus and inside that of Mars. Was it not reasonable to assume that the bulk of the earth might be comparable with that of its fellow planets, and intermediate between the bulk of Venus and that of Mars? This assumption—and, of course, it was no more than an assumption—gave the means of guessing the distance of the sun, which was concluded to be about 100 million miles.

When guesswork came to be replaced by measurement, this estimate of the sun's distance was corrected. It was found to be too large. It was amended first to 95,000,000 miles, then to 91,000,000 miles. This was subsequently found rather too small, and it is now generally thought that the sun's distance must be more than 92,000,000 miles but hardly so much as 93,000,000 miles. We have here a range of one million miles.

The problem in its present condition can now be distinctly stated. We require to determine the sun's distance accurately to within 100,000 miles, or, to speak in round numbers, we desire to determine the distance of the sun accurately to one-thousandth part of its total amount. Is such a degree of accuracy obtainable? I believe that it is. I do not say that the problem has already been solved with this precision, but an approach has been made, and enough has been done to show that the accuracy I have indicated may be attained. But this margin is not really large when we reflect on the stupendous magnitude of the sun's distance.

A favourite illustration in books of astronomy states that a journey to the sun in an express train running night and day without stopping would consume about 300 years. Before entering on such a journey it would, however, be well to recall to mind a very interesting lecture on railway accidents delivered by Sir F. Bramwell to this association a few years ago. From the figures available he showed that supposing a man made up his mind to be killed by a railway accident it would usually be necessary for him to travel day and night by express trains for 900 years before he could be quite certain of achieving his purpose. One or two return trips to the sun would no doubt suffice.

There are certain conditions which any method of measuring the sun's distance must fulfil. In the first place, it is obvious that we cannot measure the distance directly. We cannot take a tape and measure it as we would the length of a field. We are compelled to resort to indirect methods. In other words, instead of measuring the sun's distance directly, we measure something else, from which we derive the sun's distance by calculation. Whatever that something else may be, there is one obvious condition which must be fulfilled. The method by which the calculation is to be made must be absolutely unimpeachable. The measurable quantity and the sun's distance must be connected together by inexorable logic. The theory may be difficult, but it must contain no trace of ambiguity or of indefiniteness. No question of mere judgment or of estimation should be admissible. The connection between the two results must be as tight as a demonstration in geometry.

Another condition, alike obvious and important, must be specified. Whatever be the measurable quantity, be it the displacement of a planet, a lunar inequality, or the co-efficient of aberration, our measurements are subject to error. Sometimes the measured quantity will be too large, sometimes it will be too small. It is necessary to have an organised plan of symmetrical measurement, so that the number of measurements which are too great shall be as nearly as possible equal to the number of measurements which are too small. This condition is secured by forethought in arranging the details, and by vigilant suspicion of error from every conceivable scurce. The observations or measurements can then be purged from error by the well-known method of taking the mean. The success of this operation depends upon the number of observations that have been accumulated. It is, therefore, desirable that any proposed method of finding the sun's distance should admit of repeated application. Once we are assured that the observations contain no predisposition to be all too large or all too small, the mean will afford a result vastly more accurate than the original observations. It will do more than this—it will tell us not only what the result is, but how far that result is entitled to our confidence.

Let me venture on an illustration to show how accuracy may be obtained from the mean of inaccurate results. Suppose the question were to be asked this evening—What o'clock is it? and suppose that every lady and gentleman were at the same moment to look at their watches, we should have, I suppose, a thousand watches or so brought to bear on the question. Perhaps I am not wrong in supposing that, if the trial were made, the thousand watches would exhibit some degree of variety. Some, no doubt, would be right, some would be a minute or two wrong, some, perhaps, would be five minutes wrong, or even more. But though there may be a general tendency in watches to be wrong, I believe no one can assert that as a whole they exhibit any particular preference to being fast rather than slow. There are, perhaps, some hundreds of watches in the

room more or less fast, and there are probably an equal number of hundreds more or less slow. This is precisely the state of affairs that every astronomer likes. He would, under such circumstances, tolerate even watches that were very far wrong. It is quite possible that one or two of the watches present may have stopped altogether; they were not wound last night, or the spring is broken. Shall we then exclude such watches when we proceed to take the mean? It is unnecessary to do so. Even if a watch were five or six hours behind time it would only make the mean slow by about one-third of a minute, and in all probability this would be compensated by some other watch several hours too fast. The principle is sufficiently obvious. Each watch represents a more or less accurate attempt to tell the time. There is no particular bias for the watches to be fast rather than to be slow, and the greater the number the more accurate will the mean be.

The moral is obvious. If we wish to determine the sun's distance the method employed must admit of a very large number of measures being made. [About] half will be too large, [about] half will be too small, and the mean of all will afford a result which may be relied on. The various considerations I have brought forward may be considered to merge in the general condition that any proposed method must admit of the determination being made to within a thousandth part of its total amount. With this canon of criticism I shall briefly review the various methods in use, and in doing so I am glad to acknowledge how much I have profited by the labours of Mr. Gill, her Majesty's Astronomer at the Cape of Good Hope, who has, with characteristic energy, devoted himself to the discussion of this problem.

(*To be continued.*)

THE MORALITY OF HAPPINESS.

By Thomas Foster.

CHAPTER IV.—RIGHT AND WRONG.

(*Continued from page* 167.)

IN its scientific aspect, then, as indicated by processes of evolution, conduct is good in proportion as it tends to increase the quantity and the fulness of life, bad in proportion as it exerts a contrary influence. Conduct may tend to increase life in its fulness directly or indirectly, proximately or remotely; and again conduct may in one aspect increase while in another aspect it may diminish the fulness and quantity of life: but our definition of good and bad conduct is not affected by such considerations. Just as a knife may be a good knife for cutting bread and a bad knife for cutting wood, just as a business transaction may be good in relation to some immediate purpose yet bad when remoter effects are considered, so can we truly apply to conduct the terms *good* and *bad* in reference to one set of considerations even though we may have to invert the terms when conduct is considered in reference to another set of considerations. But always, in its scientific aspect, conduct is to be regarded as good where it increases life or the fulness of life, and bad where it tends the contrary way.

When we separate conduct ethically indifferent from conduct in its strict ethical aspect, it is convenient to substitute for the words *good* and *bad* the words *right* and *wrong*. But the change is slighter than at first sight it appears. Indeed the more carefully the question of rightness or wrongness,—the question of *duty*,—is considered, the more thoroughly does the kind of conduct judged to be morally indifferent merge into that which we regard as praiseworthy or censurable.

Taking first those parts of conduct which relate directly to the quantity or to the fulness of individual life, we find that while the terms good and bad are freely applied to them, and even the terms right and wrong, they are for the most part regarded as morally indifferent. When we say you *ought* to do this or to refrain from that, the idea of duty is often not really present, so long as the act in question relates to a man's own life or its fulness. Even when we use words of praise or censure in relation to such acts, they do not imply that a moral obligation has been discharged or neglected. The reason doubtless is that, as a rule, men need little encouragement to look after those parts of their conduct which affect themselves and their own interests. For it may be observed that where it is likely there may be want of due care or wisdom in such matters, there we find distinct exceptions to the general rule just indicated. So far as quantity and fulness of life are concerned, the man who crosses a crowded thoroughfare carelessly, he who neglects his business, and he who wears insufficient or unsuitable clothes in cold and wet weather, act with as little propriety in their adjustments, as is shown by the man who steadily drinks intoxicating liquors. But while none preach such duties as caution in street crossing, prudence and energy in business, and care about clothing, at least as duties morally obligatory, quite a number of persons preach against steady and heavy drinking as against a moral offence. The Bible indeed does not, though it has many a word of advice against wine-bibbing; yet even in the Bible we find evidence of the early existence of total abstainers, and it is altogether unlikely that those ancient Blue-Ribbonists omitted to recognise sinfulness in all who did not share their views and follow their practices. Here we find evidence of the law of moral philosophy that a system of ethics, with recognition of moral rightness and wrongness, only begins to be formed where the best conduct (so far as fulness of life is concerned) runs the chance, for whatever reason, of being neglected, and inferior conduct followed. In this case, the best conduct is apt to be neglected because the increased fulness of life to which it conduces is more remote than the temporary increase of life fulness to which inferior conduct tends.

Yet speaking generally it may be said that as Mr. Herbert Spencer puts it,—"The ethical judgments we pass on self-regarding acts are ordinarily little emphasised; partly because the promptings of the self-regarding desires, generally strong enough, do not need moral enforcement, and partly because the promptings of the other self-regarding desires, less strong, and often over-ridden, do need moral enforcement."

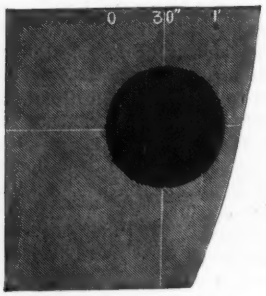
When we turn to the life-regarding actions of the second class, those which relate to the rearing of offspring, we no longer find the words good and bad, right and wrong, used with doubtful meaning. Here the question of duty is clearly recognised. The conduct of parents who by neglecting to provide for their children's wants in infancy, diminish their chances of full and active life, or of life itself, is called bad and wrong not solely or chiefly because it is not favourable to the increase of life, but as open to moral censure. In like manner, men blame as really wrong, not merely unwise or ill-adjusted, such conduct as tends to make the physical and mental training of children imperfect or inadequate.

Still clearer, however, is the use of the words right and wrong as applied to conduct by which men influence in various ways the lives of their fellows. Here the adjustments suitable for increasing the fulness of individual life

XUM

VAST SUN-SPOTS, &c. (*See opposite page.*)

Dec. 6, 1882, 2.25 P.M.



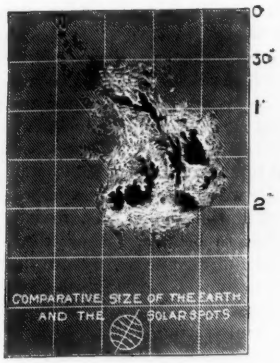
Transit of Venus, 1882.

Apparent Size and General Appearance of Venus, as seen projected on a Screen.

Aperture, 3 inches. Focal length, 45 inches. Power 80.

(The "Ligament" was glimpsed as a dusky band. Did not observe any central bright spot.)

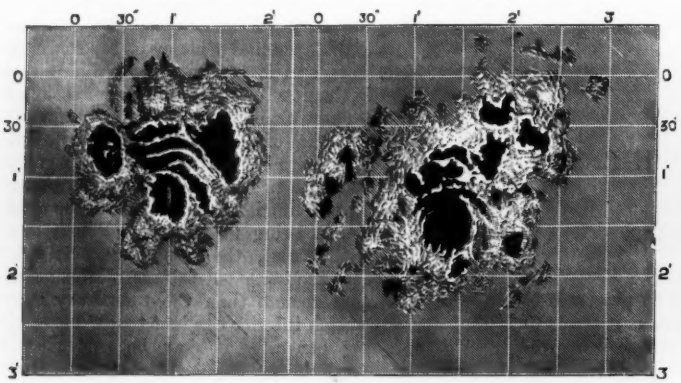
April 19, 1882, 3.30 P.M.



Vast Solar Spot of April, 1882 (accompanied with an intense Magnetic Storm).

Extreme length, 2′ 0″. Mean breadth, 0′ 45″.
∴ Area = ± 1,093,500,000 square miles.

Nov. 15, 1882, 10.47 A.M.

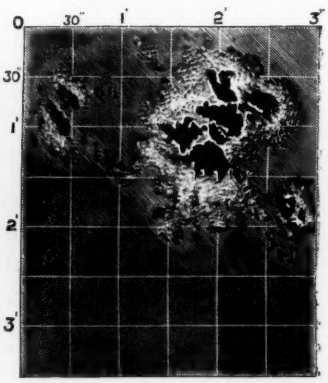


Extreme length, 2′ 12″
Mean breadth, 1′ 30″
∴ Area = ± 2,405,700,000 square miles.

Nov. 19, 1882, 10.20 A.M.

Extreme length, 3′ 10″
Mean breadth, 1′ 30″
Area ± 3,462,750,000 square miles!

Nov. 21, 1882, 10·50 A.M.



Extreme length, 2′ 0″
Mean breadth, 1′ 30″
∴ Area ± 2,187,000,000 square miles.

Huge Solar Spot of November, 1882 (accompanied with an intense Magnetic Storm).

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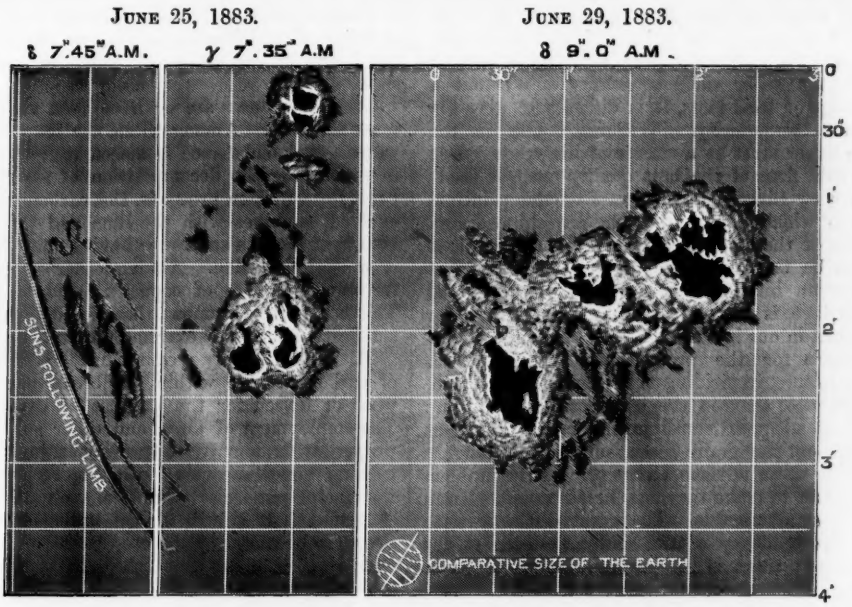
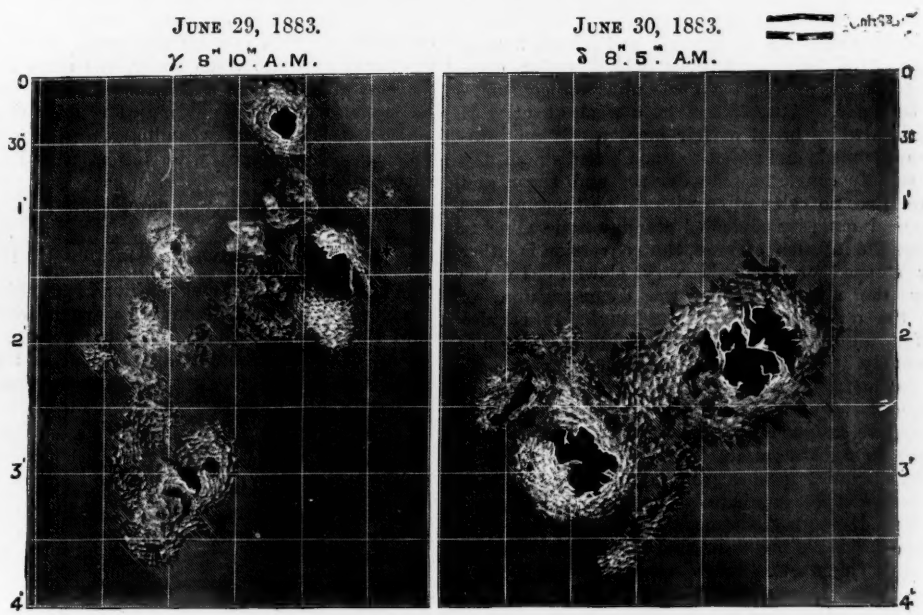
or for fostering the lives of offspring (alike in quantity and fulness), are often inconsistent with the corresponding adjustments of others. The development by evolution of conduct tending to the advancement of individual lives or lives of offspring would of itself tend constantly to acts inconsistent with the wellbeing or even with the existence of others, were it not for the development (also brought about, as we have seen, by processes of evolution) of conduct tending to the increase of the quantity and fulness of life in the community. But there arises a constant conflict between tendencies to opposite lines of conduct. It is so essential for the welfare of the community that tendencies to advance the life interests of self and children should be in due subordination (which is not the same thing be it noticed, as complete subordination) to tendencies leading to the furtherance of the fulness of life in others, that rules of conduct towards others than self or children have to be emphatic and peremptory in tone. Hence it is, as Mr. Spencer justly remarks, that the words good and bad have come to be specially associated with acts which [respectively] *further* the complete living of others and acts which *obstruct* their complete living.

(To be continued.)

During fourteen days, recently, five accidents with hydraulic lifts occurred in the City, by which four persons lost their lives.





* Notice above the absence of the usual sharp distinction between Umbra and Penumbra.

VAST SUN-SPOTS.

THE Rev. Mr. F. Howlett, whose skill and success in drawing spots, shown by the method of projection described in last week's KNOWLEDGE, is well known, has favoured the editor with photographs of the great spots visible in April and November, 1882, and in June, 1883; also of the Transit of Venus, December, 1882. From his photographs the accompanying engravings have been made.

CHEMISTRY OF THE CEREALS.

By WILLIAM JAGO, F.C.S.

No. VI.—BREAD-MAKING (*Continued*).

THE fact that there is such a thing as bad bread comes home to us at times in too pertinent a fashion to permit of any doubt of its existence. Since writing the last of these articles, I have eaten, during a stay in Liverpool, some of the best bread I ever tasted—an article far

superior to any bread I have ever obtained in the South. The particular bread referred to was obtained from an ordinary baker's shop, and was light, white, of most delicious flavour, and even after keeping some days, showed not the slightest taste or smell of acidity. Without a greater knowledge of local facts, it is difficult to give the reasons for this superiority. Without doubt, in the first place, the baker must be a master of his trade. But in addition to this, flour of the best quality must be used; it is possible that Liverpool, being one of the principal English ports to which flour and grain are shipped, and these from some of the best wheat-growing countries, that better flour can be there obtained for the same price as that asked for an inferior quality where an additional charge for carriage is incurred. Be this as it may, for reasons laid down in our last article, good flour is an indispensable requisite for the production of good bread. Unfortunately nature, in yielding us the fruits of the earth, does not always yield them in the best possible condition; rain and want of adequate sunshine may cause the wheat crop to be poor and the grain damp and not thoroughly ripened. Under these adverse circumstances the problem to be faced is how to make the best bread possible from the unsound flour obtainable. The great difficulty with such flour is the tendency of the gluten to convert the starch into sugar; there is further the danger of lactic fermentation and consequent sourness.

Certain mineral substances possess the power of arresting the action of the gluten of damp flour on its starch; among these alum has been long employed by the bakers. Alum is a double sulphate of alumina and potash, having the formula $Al_2K_2(SO_4)_4$. A flour which, under ordinary circumstances, produces a bread unfit for consumption can be made by the addition of a small quantity of alum to yield a white bread of fair quality. If an infusion of bran be made and added to some boiled starch, the starch is rapidly converted into dextrin and sugar, and the mixture gives no colouration with iodine tincture; but if to a second quantity of boiled starch a little alum is added before the infusion of bran, the conversion is either altogether stopped or proceeds very slowly. In the same way the alum prevents the decomposition of starch in the operation of bread-making. It is also claimed for alum that it prevents bread becoming sour or mouldy. Both these changes, in common with ordinary fermentation, are produced by the growth of organisms, and the action of alum may be explained by stating that its presence is injurious to these bodies; that it is, in fact, to them, a poison. Hence alum also tends to prevent fermentation proper (or alcoholic fermentation), but its presence is not so inimical to these particular germs as to those producing lactic acid and ordinary mould. So far we have been dealing with the advantages resulting from the use of alum; and there is no doubt that in many ways its action is beneficial. There is, however, another side to this question, and that relates to the consequences resulting from a continual absorption into the system of alum, even in small quantities. Opinion is here divided, but an undoubted result is the production of constipation and the symptoms of ill-health following in its train. It acts in yet another manner; in very much the same fashion as it prevents the decomposition of starch during the time the bread is "put to soak," so, too, it tends to retard the digestive action of the fluids of the stomach on the bread; much of the nutriment contained in the flour being thereby wasted. It has also been asserted that bread which has been alumed retains a much larger quantity of water than it otherwise would do. Chemical analysis does not, however, bear out this statement, for on examination some alumed loaves were found to contain 43·68, and others without alum 42·78 per cent. of water. This difference is less than the variation between the limits of percentage of water in loaves of the one class only.

Other substances are or have been suggested as substitutes for alum. Among these, the least injurious is lime-water. This substance is stated to be equally efficacious in preventing the decomposition of starch; while, as the yeast fermentation proceeds, the carbon dioxide gas that is evolved combines with the lime and produces chalk (carbonate of lime), a substance having but little action on the organs of digestion. Among other proposed substitutes for alum, sulphate of copper is said to have been used. This body is so poisonous that its employment in even the smallest quantities is extremely reprehensible.

Hitherto, the only means considered for the production of carbon dioxide within the bread has been the use of yeast or leaven for the purpose of causing fermentation. The employment of alum and other substances of similar properties is simply to prevent injurious changes proceeding simultaneously with the fermentation, unsound flour being particularly liable to such alteration.

Although it is still almost universally the custom for dough to be raised by the use of yeast or leaven, there have been from time to time other methods devised than that which depends on the production of carbon dioxide gas by the decomposition of a part of the flour itself. Among these other methods the use of "baking powder" is the best known. This substance consists essentially of bicarbonate of soda and tartaric acid. If a pinch of the powder is put into some water, the same reaction takes place as when water is added to "sherbet" or "citrate of magnesia." All these mixtures contain the two bodies above-mentioned, and when moistened the tartaric acid immediately combines with the soda and liberates the carbon dioxide:—

$H_2C_4H_4O_6$ +	$2NaHCO_3$ =	$Na_2C_4H_4O_6$ +	$2H_2O$ +	$2CO_2$
Tartaric acid.	Bicarbonate of soda.	Tartrate of soda.		Carbon Dioxide.

The baking powder in making bread is mixed with the *dry* flour, which is then made into a dough with water, cut into loaves and then baked in a quick oven. The bicarbonate of soda is sometimes used alone, as the heat of the oven is competent to effect its decomposition into normal carbonate of soda and carbon dioxide thus:—

$2NaHCO_3$	=	Na_2CO_3	+	$CO_2 + H_2O$
Bicarbonate of soda.		Normal Carbonate of soda.		Carbon Dioxide.

Although not strictly connected with our present subject, it may be of service to mention, in passing, the difference between the bicarbonate and normal carbonate of soda. The above formulæ show that the carbonate contains twice as much sodium. Carbonic acid, H_2CO_3, contains two atoms of hydrogen, which require two atoms of sodium in order to displace them; when only the one atom is displaced, a bi- or half- salt is formed. Carbonate of soda is but little used for domestic purposes, as it is more powerfully alkaline and exerts an injurious action on the coats of the stomach.

Our next article will deal specially with the properties of aërated bread.

Life of the Edison Lamp.—It is stated, says the *Electrician*, that some Edison lamps in a mill at Philadelphia have lasted on an average 3,886 hours, and are still going.

POKER PRINCIPLES.

THE following specimen of American humour will amuse our readers:—

"It is a long way from the stars to poker," says the *Chicago Tribune*, "but Richard A. Proctor has made the jump, and in the September number of *Longman's Magazine* he discusses learnedly the topic, 'Poker Principles and Chance-Laws.' It might be expected of a man whose turn of mind is scientific, who reasons on hard facts and cold deductions, that he would treat even a game of poker in the same cold, calculating manner, and Mr. Proctor does this. After describing the game, for it is not yet fairly acclimatised in England, he calculates in a cold-blooded manner the chances for each different hand at poker. He finds that the total number of poker hands is 2,598,960. He then enters into an exact mathematical calculation, and finds that in these hands there may be 40 flush sequences, 624 fours, 3,744 full hands, 5,108 common flushes, 10,200 common sequences, 54,912 triplets, 123,552 two pairs, 1,098,240 pairs, and 1,302,540 other hands. With these data for starting points, he devotes page after page to abstruse calculations of chances on given hands, and finally winds up by saying that 'poker-playing generally, as a process for making money more quickly, is much improved and enlivened by a slight degree of intoxication'—a condition which seems highly problematical if the player is expected to remember the involved mathematical processes which he lays down, and certainly implies a moral laxity which makes one shudder at the mere thought. If Professor Proctor really believes that intoxication is an aid in perfecting and applying mathematical deduction, it is easy to understand why he has so often slipped up in his celestial mathematics." [My Chicagoan friend should have shown when and where; since, however, the remark about intoxication was not mine, but was made by an American writer on Poker Principles, the Chicagoan's vague idea about errors in my calculations may be taken for what it is worth—nothing.—R. P.]

"The most startling feature of Prof. Proctor's discussion is the manner in which he treats bluffing. He gravely recommends intoxication as a prime condition for playing a good or poor hand, and regards it as mathematically moral—but 'bluffing should be omitted, as practice in this department of the game is really practice in the art of lying with unchanging face, and this is an undesirable art, whatever rogues may think. The gain which can be made by skill in lying is more than matched by the loss which a reputation for such skill is sure to bring.' This, it need not be said, is a narrow English view of a purely American institution. It is not poker as Minister Schenck taught it to the court and people to which he was accredited—a boon which has made his name famous in the annals of diplomacy. It is not the kind of game played by Clay and Webster, which Mr. Proctor cites, in which after each had $2,000 on the board, Clay called Webster on an ace-high, and found his opponent had a pair of deuces, in commenting upon which the astromoner learnedly says: 'The strange part of the story is that Clay should have called, for, apart from any question whether Webster were bluffing or not, ace-high is not a hand on which to call.' Mr. Proctor will never induce those who have once come under the fascination of the game to avoid bluffing. To play the game on such a condition involves about the same degree of thrilling interest one would experience in sitting down to calculate a table of logarithms or to work out an endless length of sines and cosines."

"Unwittingly, perhaps, he has stripped the game of all that has made it so dear to the American heart. By his methods it loses all its airiness of contour, its mysterious glamour of illusion, its dashing assaults, its leadings of forlorn hopes, its thrilling surprises, its ruthless blights of expectation, its oscillations from hope to defeat, and is reduced to a mere dry, hard, cold mathematical computation, which is not even mitigated by his recommendation that the players get intoxicated. It plucks the very heart out of the game, and slaughters the mystical sphinx whose riddle at poker no man has ever yet read. If a full hand must always be beaten by fours, or two pairs by a triplet, what inducement is there to play poker? Better casino and old maid than this game emasculated of that mysterious, subtle, always-appearing-when-least-expected quality which makes it possible for a jack high to sweep the board against three kings or an ace full. No! Mr. Astronomer. Poker knows no laws and spurns all conditions. The man is its measure, and he who has the genuine poker afflatus, though he were a clown and had but a pair of deuces, would be dangerous to Mr. Proctor with a straight flush, even were he to follow his own recommendation and get as drunk as a lord. Let Mr. Proctor take his heaps of calculations and come into these Western wilds, if he wants to know the legions of possibilities that lurk in this noble game."

[As I never took a hand at poker and am not likely to, the legions of possibilities in poker will for ever remain unknown to me, except as indicated by mathematics.—R. P.]

The American Telegraph System.—A correspondent of the *Daily Telegraph* recently wired as follows from Washington:—"The Committee of the United States Senate appointed to inquire into the American telegraph system and the advisability of its acquirement by the State on the English plan met to day. The principal witness examined was Mr. Jay Gould, the eminent financier and president of the Western Union Telegraph Company, who emphatically denounced the scheme for placing the telegraphs under the control of the Government. He affirmed that Government telegraphs must always be inefficient, unstable, and subject to political influence."

Anthracene and Light.—The substance known as anthracene has been found by Dr. Tommasi to possess a new property, namely, a sensitiveness to light, which will doubtless prove of value. Anthracene on exposure to light acquires different physical and chemical properties without any change in its composition. If a cold clear saturated solution of anthracene in benzol is exposed to the direct rays of the sun, it becomes turbid and deposits crystals, which have received the name of paranthracene.—*Engineering.*

A new mountain railway—Territet Montreux-Glion—has been constructed on the shores of the Lake of Geneva. No locomotive is used, but the weight of the descending carriage is used to bring the ascending vehicle up the incline. The necessary increase of weight required is provided for by water ballast. The line is only about 750 yards long, and the height attained is 327 yards above the spot where the railway commences. The lower portion of the line has a gradient of 300 in 1,000, and the upper portion 570 in 1,000. An automatic brake has been provided, which acts in case a breakage of the rope takes place. The carriages are specially built in consideration of the gradients referred to, and are constructed to hold twenty persons each.—*Engineer.*

XUM

THE PHILOSOPHY OF MATHEMATICS.*

By Professor A. Cayley.

IN TWO PARTS.—PART I.

MATHEMATICS connect themselves on the one side with common life and the physical sciences; on the other side, with philosophy, in regard to our notions of space and time, and in the questions which have arisen as to the universality and necessity of the truths of mathematics and the foundation of our knowledge of them.

As to the former side, I am not making before you a defence of mathematics. Still less would I speak of its utility before, I trust, a friendly audience, interested or willing to appreciate an interest in mathematics in itself and for its own sake.

On the other side, the general opinion has been, and is, that it is indeed by experience that we arrive at the truths of mathematics, but that experience is not their proper foundation; the mind itself contributes something. But it is maintained by John Stuart Mill that the truths of mathematics, in particular those of geometry, rest on experience;† and, as regards geometry, the same view is on very different grounds maintained by the mathematician Riemann.

I think it may be at once conceded that the truths of geometry are truths precisely because they relate to and express the properties of what Mill calls "purely imaginary objects." That these objects do not exist in Mill's sense, that they do not exist in nature, may also be granted; that they are "not even possible," if this means not possible in an existing nature, may also be granted. That we cannot "conceive" them depends on the meaning which we attach to the word conceive. I would myself say that the purely imaginary objects are the only realities, the ὄντως ὄντα, in regard to which the corresponding physical objects are as the shadows in the cave; and it is only by means of them that we are able to deny the existence of a corresponding physical object. If there is no conception of straightness, then it is meaningless to deny the existence of a perfectly straight line. But, at any rate, the objects of geometrical truth are the so-called imaginary objects of Mill, and the truths of geometry are only true, and *à fortiori* are only necessarily true, in regard to these so-called imaginary objects; and these objects, points, lines, circles, &c., in the mathematical sense of the terms, have a likeness to, and are represented more or less imperfectly, and from a geometer's point of view, no matter how imperfectly, by corresponding physical points, lines, circles, &c.

I shall have to return to geometry, and will then speak of Riemann, but I will first refer to another passage of the "Logic." Speaking of the truths of arithmetic, Mill says (p. 297) that even here there is one hypothetical element:—"In all propositions concerning numbers a condition is implied without which none of them would be true, and that condition is an assumption which may be false. The condition is that 1=1; that all the numbers are numbers of the same or of equal units." Here, at least the assumption may be absolutely true; one shilling=one shilling in purchasing power, although they may not be absolutely of the same weight and fineness, but it is hardly necessary; one coin+one coin=two coins, even if the one be a shilling and the other a half-crown. In fact, whatever difficulty be raisable as to geometry, it seems to me that no similar difficulty applies to arithmetic; mathematicians or not, we have each of us, in its most abstract form, the idea of a number; we can each of us appreciate the truth of a proposition in regard to numbers, and we cannot but see that a truth in regard to numbers is something different in kind from an experimental truth generalised from experience. Compare, for instance, the proposition that the sun, having already risen so many times, will rise to-morrow, and the next day, and the day after that, and so on, and the proposition that even and odd numbers succeed each other alternately *ad infinitum*, the latter, at least, seems to have the characters of universality and necessity. Or, again, suppose a proposition observed to hold good for a long series of numbers, 1,000 numbers, 2,000 numbers, as the case may be, this is not only no proof, but it is absolutely no evidence, that the proposition is a true proposition holding good for all numbers whatever; there are in

* Abstract of that portion of Professor Cayley's Address before the British Association at Southport, which relates to the philosophy of Mathematics and to certain recent ideas respecting non-Euclidian Geometry and space of more than three dimensions.

† "It remains to inquire what is the ground of our belief in axioms, what is the evidence on which they rest. I answer, they are experimental truths, generalisations from experience. The proposition 'Two straight lines cannot enclose a space,' or, in other words, two straight lines which have once met cannot meet again, is an induction from the evidence of our senses." But I cannot help considering a previous argument (p. 259) as very materially modifying this absolute contradiction. After inquiring, "Why are mathematics by almost all philosophers . . . considered to be independent of the evidence of experience and observation, and characterised as systems of necessary truth?" Mill proceeds as follows:—"The answer I conceive to be that this character of necessity ascribed to the truths of mathematics, and even (with some reservations to be hereafter made) the peculiar certainty ascribed to them, is a delusion, in order to sustain which it is necessary to suppose that those truths relate to and express the properties of purely imaginary objects. It is acknowledged that the conclusions of geometry are derived partly at least from the so-called definitions, and that these definitions are assumed to be correct representations, as far as they go, of the objects with which geometry is conversant. Now, we have pointed out that from a definition as such, no proposition, unless it be one concerning the meaning of a word, can ever follow, and that what apparently follows from a definition follows in reality from an implied assumption that there exists a real thing conformable thereto. This assumption in the case of the definitions of geometry is not strictly true; there exist no real things exactly conformable to the definitions. There exist no real points without magnitude, no lines without breadth, nor perfectly straight, no circles with all their radii exactly equal, nor squares with all their angles perfectly right. It will be said that the assumption does not extend to the actual but only to the possible existence of such things. I answer that, according to every test we have of possibility, they are not even possible. Their existence, so far as we can form any judgment, would seem to be inconsistent with the physical constitution of our planet at least, if not of the universal (*sic*). To get rid of this difficulty and at the same time to save the credit of the supposed system of necessary truth, it is customary to say that the points, lines, circles, and squares which are the subjects of geometry exist in our conceptions merely, and are parts of our minds, which minds, by working on their own materials, construct an *à priori* science the evidence of which is purely mental and has nothing to do with outward experience. By howsoever high authority this doctrine has been sanctioned, it appears to me psychologically incorrect. The points, lines, and squares which any one has in his mind are (as I apprehend) simply copies of the points, lines, and squares which he has known in his experience. Our idea of a point I apprehend to be simply our idea of the *minimum visible*—the small portion of surface which we can see. We can reason about a line as if it had no breadth because we have a power which we can exercise over the operations of our minds—the power, when perception is present to our senses or a conception to our intellects, of attending to a part only of that perception or conception instead of the whole. But we cannot conceive a line without breadth—we can form no mental picture of such a line; all the lines which we have in our mind are lines possessing breadth. If any one doubts this, we may refer him to his own experience. I much question if any one who fancies that he can conceive of a mathematical line thinks so from the evidence of his own consciousness. I suspect it is rather because he supposes that unless such a perception be possible, mathematics could not exist as a science—a supposition which there will be no difficulty in showing to be groundless."

the Theory of Numbers very remarkable instances of propositions observed to hold good for very long series of numbers, and which are nevertheless untrue.

I pass in review certain mathematical theories. In arithmetic and algebra, or say in analysis, the numbers or magnitudes which we represent by symbols are, in the first instance, ordinary (that is, positive) numbers or magnitudes. We have also in analysis and in analytical geometry negative magnitudes; there has been in regard to these plenty of philosophical discussion, and I might refer to Kant's paper, "Ueber die negativen Grössen in die Weltweisheit" (1763); but the notion of a negative magnitude has become quite a familiar one, and has extended itself into common phraseology. I may remark that it is used in a very refined manner in bookkeeping by double entry.

But it is far otherwise with the notion which is really the fundamental one (and I cannot too strongly emphasize the assertion) underlying and pervading the whole of modern analysis and geometry—that of imaginary magnitude in analysis and of imaginary space (or space as a *locus in quo* of imaginary points and figures) in geometry. I use in each case the word "imaginary" as including real. This has not been, so far as I am aware, a subject of philosophical discussion or inquiry. As regards the older metaphysical writers this would be quite accounted for by saying that they knew nothing, and were not bound to know anything, about it; but at present, and considering the prominent position which the notion occupies, say even that the conclusion were that the notion belongs to mere technical mathematics, or has reference to nonentities in regard to which no science is possible, still it seems to me that (as a subject of philosophical discussion) the notion ought not to be thus ignored; it should at least be shown that there is a right to ignore it.

Although in logical order I should perhaps now speak of the notion just referred to, it will be convenient to speak first of some other quasi-geometrical notions; those of more-than-three-dimensional space, and of non-Euclidian two- and three-dimensional space, and also of the generalised notion of distance. It is in connection with these that Riemann considered that our notion of space is founded on experience, or rather that it is only by experience that we know that our space is Euclidian space. It is well known that Euclid's twelfth axiom, even in Playfair's form of it, has been considered as needing demonstration; and that Lobatschewsky constructed a perfectly consistent theory, wherein this axiom was assumed not to hold good, or say a system of non-Euclidian plane geometry. There is a like system of non-Euclidian solid geometry.

My own view is that Euclid's twelfth axiom in Playfair's form of it does not need demonstration; that it is part of our notion of space, of the physical space of our experience—the space, that is, which we become acquainted with by experience, but which is the representation lying at the foundation of all external experience. Riemann's view before referred to may, I think, be said to be that, having *in intellectu* a more general notion of space—in fact, a notion of non-Eucludian space, we learn by experience that space (the physical space of our experience) is, if not exactly, at least to the highest degree of approximation, Euclidian space. But suppose the physical space of our experience to be thus only approximately Euclidian space, what is the consequence which follows? Not that the propositions of geometry are only approximately true, but that they remain absolutely true in regard to that Euclidian space which has been so long regarded as being the physical space of our experience.

(*Part II. in our next.*)

COMMAS AND COLONS.

THERE is a further argument against the Brighton theory of parenthetic commas round adverbs, which will reveal itself if you merely write instead of "He insulted me, and therefore I went away," "He insulted me, and I went away in consequence," or "therefore." Who would think of putting a comma before "therefore" in that case? And in either position who would think of speaking it *commatically*, which is often, if not always, the best rule for punctuation?

I by no means disregard the distinction between colons and semicolons myself; but I cannot shut my eyes to the fact that much better writers than I am are far from consistent in their use of them; and I cannot find, or hardly imagine, a case where the substitution of one for the other would make any difference in either the real or the apparent sense. On the other hand, cases are constantly occurring where one would like a smaller comma—or a larger one, as the case may be.

I thank you for your protection as to the evidently missing comma in my former letter, which I was sure that some "F. R." would be down upon. His criticism on my ambiguous "it" proves that he is one of those who think that if you can put a wrong or absurd meaning into somebody else's writing the somebody else must have meant it.

EDM. BECKETT.

THE FACE OF THE SKY.

FROM SEPT. 28 TO OCT. 12.

BY F.R.A.S.

THE sun will be examined, as usual, for indications of disturbance on every clear day, and the Zodiacal light may be looked for in the East before sunrise. Map X. of "The Stars in their Seasons," gives a picture of the Night Sky during our prescribed period. Mercury sets after the sun to-night, but comes into inferior conjunction with him on October 7, after which Mercury is a morning star. He is most indifferently placed for the observer. Venus is practically invisible. Mars still souths during the morning hours, but sooner and sooner every night, between the N.E. and the E.N.E. points of the horizon. He still presents the appearance of an exaggerated red star. Jupiter now rises before midnight. He is pretty close to Mars in the sky. No phenomena of his satellites occur at times suited to the ordinary observer during the next fourteen days. Saturn, some 3° or 4° North of Aldebaran, rises soon after 8 p.m., and is getting nightly into a more favourable position for the observer. The remarks which we made a fortnight ago with regard to Uranus and Neptune are still applicable. The moon is 26·9 days old at noon to-day (September 28), and 0·3 days old on October 1; her age being quite evidently 11·3 days at the same hour on October 12. Hence she will be fairly well placed for the observer during a good deal of the time to which these notes refer. No occultations of stars happen during the period which they cover. The moon is in Sextans at noon to-day, re-entering Leo about midnight. She quits this constellation for Virgo about 4 a.m. on the 30th. She occupies until about 2 p.m. on Oct. 3 in crossing Virgo, and at that hour enters Libra, which she takes until between 5 and 6 p.m. on the 5th to traverse. Then she arrives at the narrow northern part of Scorpio, over which she passes before 4 o'clock the next morning. From that hour until nearly midnight on the 7th she is traversing the southern part of Ophiuchus; but she then crosses its boundary into Sagittarius. Her path through the last-named constellation takes her until 11 a.m. on the 10th to describe. About 3 o'clock on the morning of the 4th she crosses into Aquarius. There we leave her.

NORTH AMERICAN INDIANS AS RAILWAY NAVVIES.—From fifty to seventy Winnebago Indians are employed on the Omaha Railway as labourers. They are eager to obtain the work, to which they consider they have a claim, and make excellent labourers, doing a better day's work than the Chinese.—*Engineering.*

Editorial Gossip.

The British Association meeting makes large claims upon our space this week, and will do so for the next two or three weeks. We propose, when the rest of Professor Cayley's address has appeared, with its amusing references to non-Euclidian space and fourth, fifth, and yet higher dimensions in space, to comment on these conceptions, which certain non-mathematical writers, in their amazement at such profundities, have described in the daily press in terms which would be scarcely justified if applied to Newton's greatest discovery.

In writing my "Gossip" for last week I referred to that part of Sir George Airy's library which he assigned to paradoxists,—Reddie, Parallax, Hampden, *et hoc genus omne*. I was in doubt as to the actual title that he gave to this part of his library, and having written "My Lunatic Asylum," changed the title into "My Asylum for Lunatics," but suppose I failed to delete the first "Lunatic," for the rather tautological title "My Lunatic Asylum for Lunatics" appeared instead of what I intended.

Truth gives me comfort. Several reviewers found my article on Poker Principles in *Longman's Magazine* too abstruse. *Truth* says it only shows that two and two make four. Yet *Truth* condescends to borrow my table of poker hands as "curious." As a considerable part of the article is occupied with the reasoning by which the table was established, this is rather incongruous on Mr. Labouchere's part. Perhaps he objected to some remarks of mine on the Bishop-Labouchere row,—in which, however, I take no manner of interest personally. I only objected to a writer, whose authority on actors and princes and dukes and actresses and circuses may very likely be high, undertaking to define the limits of mental influence, where men like Dr. Carpenter and Professor Barrett have been unable to formulate an opinion.

He goes on to ridicule my remark that "bluffing" at Poker is not desirable,—he makes me say "immoral" which is unnecessary,—because "the gain which can be made by skill in lying is more than matched by the loss which reputation for such skill is sure to bring." "This is about as ridiculous," he says, "as [it would be] to suggest that whist should be played without even finessing because it is a deception practised upon the adversary." My critic appears to confound "finessing" with "underplay,"—a very different thing. For in the great majority of cases, "finessing" is not a deception practised upon the adversary; "underplay" is. However, his argument is not affected by his blunder. But no one would think of regarding "underplay" as wrong, any more than he would regard feints in fencing or single-stick as immoral. Bluffing which is lying for money is a different kind of trick altogether; yet not immoral if gross and greedy gambling is not immoral. But my point was the inexpediency of frequent bluffing, regarded with direct reference to the game itself. To be effective, bluffing should only be resorted to occasionally. As to the expediency of "underplay," Clay has spoken pretty strongly, objecting to it for the very reason indicated by myself in regard to bluffing, that in the long run it does not pay. Probably Clay knew as much about whist as Mr. Labouchere: at any rate I do not find that Clay ever confounded "finessing" with "underplay" (excepting in reference to those special cases where a finesse involves underplay).

"Let Knowledge grow from more to more."—Alfred Tennyson.

Only a small proportion of Letters received can possibly be inserted. Correspondents must not be offended, therefore, should their letters not appear.

All Editorial communications should be addressed to the Editor of Knowledge; *all Business communications to the* Publishers, *at the Office, 74, Great Queen-street, W.C.* If this is not attended to delays arise for which the Editor is not responsible.

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HIGH WHEELED TRICYCLES *v.* LOW WHEELS.

[938]—In spite of what Mr. Browning says further in last week's Knowledge upon this question, I still hold the conviction that high wheels, *i.e.*, wheels of 50 in. diameter, and when geared up ranging down to say 46 in. with ordinary machines must, and do, give better results as regards general ease of propulsion, and easily acquired and maintained speed, than wheels of the diminutive size he advocates.

It must be remembered that Mr. Browning says he now rides a 38-in. wheeled tricycle, and proposes to have a 36-in. wheel next year.

There are, I venture to say, very few practical riders of any powers that will not agree with me that so small a wheel is a mistake.

In the first place the pedalling must be very rapid to get up any speed at all, and there is a limit to gearing up.

Mr. Browning quotes Mr. Nixon as one of his supporters, but I believe I am right in stating that Mr. Nixon rides machines of 48-in. wheels.

At any rate, I should be very glad to "take him on" if he will ride a machine with 36-in., or even 38-in. wheels of any ordinary pattern. I should then prefer to have a muddy or rough Macadam road, and although Mr. Nixon is undoubtedly a more highly-trained rider than I should ever take the trouble to be, I have very little fear of the result. I should ride my 50-in. Imperial Club light roadster that I have now ridden over 3,000 miles, geared up to 60 inches.

I should bar the "Humber," which is for some reason an exception, but even the "Humber" will, not, I imagine, produce such good results if the wheels fall below, say, 42 in.

Mr. Marriott, who is a remarkable rider, is said to have ridden a 40-in. in the recent fifty miles championship. He did not, however, make such fast time as Lowndes did last year with 48-in. wheels.

At hill-riding, I am satisfied that small wheels are at a decided disadvantage, and as I live at the foot of Muswell-hill, I am in a good position to speak from practical experience. I have ridden this hill from bottom to top without stopping, in all over 50 times, and always with a 50-in. or 52-in. wheeled machine. I once tried hard with a 44-in. machine, but could not get up, but directly afterwards I mounted the hill with a machine of exactly the same make —a Cheylesmore—weighing about the same, with 52-in. wheels. I believe the reason for this is that the small wheels have to go round so much more frequently that the influence of the dead point comes in so often as to impede progression.

I have now in my stable a Sociable with 40-in. wheels, and the riding of this machine, although it is geared up to 47 in., is torture as compared with my 50-in. geared up to 60 in. It is harder work, even with a strong rider in company, and to get up a pace equal to that I can keep up without effort on the 50-in., the pedalling has to be so fast as soon to become fatiguing. It is only by dint of very rapid pedalling that a speed of some eight miles an hour can be attained, while with the 50-in., very slow, easy pedalling will achieve this very moderate pace.

I have recently been riding an "Otto" with 56-in. wheels, and the roll of these big wheels was truly delightful, carrying one over the rough roads without noticing the loose places. An "Otto" with 60-in. wheels, geared level, was ridden up Muswell Hill the other

day, and I will venture to say that it would be a task of no little difficulty to get a 36-in. or 38-in. wheeled geared-up machine up this gradient.

Exceptionally small wheels may do very well on the smooth cinder racing-track, but they will certainly not answer so well as reasonably high wheels for ordinary road riding.

I should like to see two men of equal powers who have never been on a tricycle mounted—the one on Mr. Browning's pet machine, and the other on my pet machine—and I would "bet my bottom dollar" on the result, but for the fact that I never bet at all.

Fancy the appearance of a tall man on a very small Shetland pony! *Verb. sat. sap.!* SIGMA.

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[939]—As one deriving much enjoyment in the use of the tricycle, permit me to say that I think the advocates for high, as also for low wheels, are endeavouring to prove too much. As with horses, so with tricycles, there is no particular height suited to every weight and build of rider. There is much room for the exercise of judgment in the matter of height of wheel and kind of machine suited to each individual rider.

There are, however, a few simple rules which may, and certainly should, be applied to the selection and use of tricycles, whether of high or low wheels.

(1) To economise power it is essential that the saddle-seats, being only suited to invalids, should be well over, or even somewhat in advance of the pedals.

(2) The centre of gravity should be as low as possible. This is obtained by fixing the pedals as near the ground as may be sufficient to clear inequalities and loose stones on the surface of the road. I find 4 in. sufficient clearance.

(3) The weight should be pretty evenly distributed on all the wheels.

I could add to the list, but I spare you, and with the foregoing secured the novice would be started on the right "track," and would soon learn to ride with ease and elegance—both at present conspicuously absent from the great majority of tricycle riders.

W. H. FRANCE.

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[940]—It has been with interest that I have read these articles, especially as they seem to have been written for people like myself, who are far from strong. To those who object to small wheels and have not the power to propel the large ones up hill, I would recommend the omnicycle. Its wheels are 50 in. It has three gears. Speed for ordinary work, medium for rough or heavy roads, and power for up-hill work; balance gear and free running wheels, both of which are an immense blessing. It runs smoothly and evenly, and, being a lever action, it has no dead points and can be started with ease, even half way up a hill. I run mine in hilly country, and find it everything that can be desired.

JOHN ALEX. OLLARD.

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[941]—"Sigma's" article on tricycles, in No. 98 of KNOWLEDGE, is valuable, as recording the opinion of a rider—and one of our best riders too—of a 50 in. wheeled tricycle geared up to 60 in. But has "Sigma" ever *thoroughly tried* a small wheel (say 40 in.) geared up to the same figure? I fancy not.

One paragraph of the article under question would almost lead any one who did not know the writer to imagine that he was not a rider at all. I refer to where he says: "Nothing wearies so much as rapid pedalling, and nothing looks so ungraceful; and it is obvious that with small wheels the pedalling must be very rapid indeed to get any pace at all." Now, even the greatest novice at tricycling knows that a small wheel can be geared up to any size, and that, consequently, the feet revolve slower proportionately as the size increases.

For myself, I very much prefer a small wheel, and this after experience with eleven or twelve machines, varying in size from 54 inches to 40 inches, geared in different ways. My present mount—a 40-inch "Shadow," geared to 52 inches, and weighing only 46 lb.—I ride *with ease with 4½-inch cranks*, the usual length of a tricycle crank being 5½ inches.

The advocates of large wheels talk about the loss of power and bumping on bad macadam roads which the riders of small wheels have to put up with, but it is my experience that for every mile of bad—*really bad*—road met with in the course of a year, I get hundreds of miles of good road, so we don't lose much there.

I believe I am right in saying that Mr. Browning does not attempt to advise on machines for racing men, but for those who ride for *pleasure*, and wish to do their forty or fifty miles a day with comfort and ease; hence his advice to gear low.

Thanking you for opening your valuable columns for the discussion of the construction of the "Coming Tricycle," I am, &c.,

S. H. R. SALMON,
Hon. Sec. London Tricycle Club, T.U.

THE "SUN AND PLANET" BICYCLE.

[942]—In reply to Mr. Browning, my machine weighs 45 lb. exactly. It is heavier than it should be, the forks and backbone being very wide. But I suppose, as the difference in weight of bicycles can only amount to a few pounds, it does not affect the speed much, as it must with tricycles when it comes to 30 lb. or so. I find the machine goes much easier than the Cheylesmore and other tricycles I have been in the habit of riding. I get a beautiful pace out of it, and it is curious that the slow motion of the seat made me feel at first that I *must* be on a "50-inch"—this was rather unpleasant until I overcame the illusion.

When dangers thicken around you, on a small machine nothing is easier than to drop back at once to the ground, in a position of perfect safety, and ready to run the machine anywhere. This could not be done with a tricycle.

The machines are very neat and well made—like engine work—and I can recommend them as the farthest advance I know of in the direction of speed and safety.

Really the fallacies in "Sigma's" remarks seem too plain to need pointing out, but one is that he seems to have an idea Mr. Browning is speaking of *geared-up* wheels. S. J.

NAKED-EYE SUN-SPOT.

[943]—Once more I take up my pen to record the appearance of another spot on the sun, so large as to be easily visible to the naked eye.

The first time I saw it in that way was on Wednesday evening last, September 12, when the sun was setting like a great ball of copper through the mist of the horizon, the spot being very distinct, situated a little below the centre of the disc; and each day since then I have seen it several times through smoked glass.

It is a spot that I first saw with my telescope on Saturday last, September 8, when it was just coming on at the eastern edge of the sun's disc, and consequently appeared very much elongated and foreshortened, and, as far as I could then judge, it was only one spot, but on examining it again with the telescope yesterday, I found it was really composed of two almost entirely distinct spots, of about equal size, placed so close together that the extreme edges of their penumbras at one point appeared to just touch each other. Each spot individually is very large, and so taken together they constitute a tremendous disturbance; and then there are several other smaller spots scattered about the sun's disc, so that I think we may safely say that there is some very great agitation going on there again now, as there has been so many times during the last twelve months. EXCELSIOR.

A CURIOUS PHENOMENON.

[944]—The remarkable phenomenon which Mr. Noble described in No. 98 of KNOWLEDGE was also witnessed by me in Liverpool on Aug. 29, at 12 h. 40 m. a.m. I had just been looking at Saturn, when, for the first time, I saw a bright divergent cone of light about 7° above the horizon; the entire length of the cone was about 5°. The apex or nucleus displayed such a degree of concentration that I thought it was the planet Jupiter. I turned my telescope, a 2-inch, armed with a power of 30, on the point where the apex should be (it was now obscured by a cloud), with the expectation of being able to unravel the mystery, but was disappeinted, as the cloud was too dense. I then ran my telescope along the major axis of the cone, and the field of view was so faintly illuminated that the brightest part could hardly be said to equal the *lumière cendrée* seen under similar conditions. It gradually faded from view, after having been visible for thirteen minutes. I continued watching the part of the heavens where it had disappeared, with the confident hope that it would return, but was at last obliged to give it up, as a great bank of clouds precluded all further observation.

It could hardly have been an auroral streamer, as the point where it appeared is 67° east of North. W. K. BRADGATE.

Sept. 15.

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[945]—The phenomenon seen by Captain Noble may have been the inverted cone of light proceeding from an ironfounders' cupola, visible in slight haze. A line drawn from the observatory in an E.N.E. direction passes nearly through Ashford, in Kent. I do not know that place, but my map of Kent shows it to be an important railway junction, with an "engine depôt." It is probable that at such a place there is a cupola, and that melting might be going on as late as 10.35 p.m. The light is intensely brilliant during the latter part of the operation of melting, when the white-hot metal or slag is left at the bottom of the furnace, and might, I think, be seen, under favourable circumstances, as far off as Ashford (twenty-six miles). That it was a distant object is evident from the sketch. Various causes might account for the sudden extinction.

J. M. COATES.

XUM

THE "HUMBLE SOLDIERS."

[946]—May I (perhaps in atonement, being one of the "point-pepperers") give what seems to me to be a good reason for the elimination of the commas in the sentence, "He called for help; and, therefore, I went to him."

I would submit to the writer in the *Brighton Herald* that the word "therefore" is here used as equivalent in sense to the words "that was why," or "for that reason;" and surely he would not insert the commas in either of the following sentences:—

"He called for help; and *that was why* I went to him."

"He called for help; and *for that reason* I went to him."

COMPOSITOR.

LETTERS RECEIVED, AND SHORT ANSWERS.

F. M. DIPLOCK. I do not see, either, that the comma is out of place. But it is well to note that the word "Why," in the sentence, is not a question *per se*, as many imagine.—W. B. All the planets turn the same way round as the sun does, except Uranus, and probably Neptune. This way round is that also in which they all, without any exception at all, travel round the sun.—W. O. DAWSON. Article in type.—W. LAWRENCE. If Mr. John Hampden had dictated your letter you could not more completely have caught his style. Permit me to congratulate you. I thought him unique: now I see I was mistaken. If I have "failed to convince a single individual who was in doubt about it" ("it" being the earth's rotundity) I have failed in what I have never attempted to do. Mr. Browning makes excellent spectacles, but he has failed to make a single individual see who chanced to be born blind. (He has not tried, that I am aware, but that only makes the parable better; for neither have I tried to make those see with the mental eye who are mentally blind.)—A. M. Those passages are given in a little volume, published by Messrs. Appleton, New York, containing the speeches (made and not made) at the dinner given in honour of Mr. Herbert Spencer's visit to America. By the way, I am inclined to wonder what would be Mr. Spencer's deliberate opinion on the subject of dinner-giving as a way of indicating esteem and admiration.—D. C. S. (1.) Probably a harmless, but still more probably a useless recipe. You would have to buy of the person who seems so generously to offer the recipe. Note his prices, and recognise rascality. (2.) Yes; smoke-abatement an important subject; *ex fumo dare lucem*—if it could but be managed.—SENEX (1.) Not that "silent lightning" is never seen in daytime, but why it is not so often seen in daytime as at night (your note on this subject slightly shortened, will appear). (2.) One cannot directly compare the intensity of waves of light, sound, and water. (3.) Reflection of rainbow can be seen.—COSMOS. Neither the longitude of perihelion nor the inclination of major axis is assumed. The initial assumptions are at once less simple and more tentative. The subject is one for an article, not for a Short Answer.—J. W. WILSON. If we were certain the mass of the two stars together equalled the mass of the sun and earth, we could infer from the period of revolution the precise distance. But in every case yet dealt with we have either had no such knowledge, or have known that the case is quite otherwise. In my "Other Worlds than Ours" the points involved in the problem are fully discussed.—E. T. L. The papers on the Moon began with Vol. III., and the reference map appeared with the first of them.—W. S. BOSCAWEN. I do not know where the rising and setting of stars visible in Babylonia as early as 2,000 B.C. can be obtained. In my library star atlas, and in the maps of my gnomonic atlas, are the longitude and latitude lines, by which correction can be made in the case of any star, for the effects of precession. The star must be carried westwards along its latitude parallel at the rate of 5° for about 359 years of past time. This serves for the vernal equinox also.—M. J. HARDING. I am sorry more copies of the Index were not printed. But a copy was sent to every subscriber (as distinguished from buyers). I myself much prefer the plan of issuing the index with the paper. But the publishers saw reasons for adopting a different plan.—D. E. SAMUEL. The nasal twang of the true Yankee or New Englander has been derived from Puritan ancestors. The Roundheads so spoke. But chronic catarrh may now have something to do with the matter.—E. S. B. Thanks for kind letter. Hints noted. With regard to the "sneak" as you justly call him referred to in article on "Social Dynamite," I have never been in doubt who he was, since Mr. Lockyer disclaimed all knowledge of the matter. I have no doubt either, that after reading what I there wrote, Mr. Lockyer himself could form a tolerably shrewd guess on the subject. He has not however thanked me yet for enabling him to see what a treacherous "friend" that particular person was.—E. F. T. *La Nature* might suit you.—M. C. I think the flat earth folk would deny the validity of the spirit-level proof.—W. G. KNOWLES. Read Darwin's book on "Mould and Worms."—GEORGE JORDAN. Cannot further try to explain matters to one unfamiliar with even the elements of science. Have already given to you much more time and attention than you could expect.—W. W. F. The whole column of water.—H. ASKEW. (1) Some nonsense about the holy numbers three and seven. I forget what. (2). It is difficult to get in all desirable subjects.—J. G. FISHER. The printers arrange that matter of month, day, year. Your letter sent to printers. —INQUIRER. If you will state in a letter the rules for the four possible cases, will publish: but a single case out of four would only perplex readers.—T. MAUDE. There are differences of atmospheric pressure in different latitudes, and these differences obviously depend on latitude; but they are certainly not proportional to the rate of terrestrial rotation. The pressure and density are greatest in the sub-tropical zones.—W. B. No one would think of extracting square root by logarithms, when only two or three digits are wanted.—UNCERTAIN. I fear those metaphysical speculations would have no interest for readers. As to your swearing, *I* can have no manner of objection. It may be as you suggest, "an unco' relief to a body." I fancy it is like dram-drinking though, and that the round oath which relieves your feelings now will require to be replaced by a stronger one hereafter. My own personal objection to swearing is that no matter what form of oath one adopts, one becomes conscious of temporary misuse of reason in employing it. Whether you swear by "thunder," or wish you may be "dad-slapped," or call what offends you a "dodgasted" noun of some sort, or prefer something more genuinely profane, comes to much the same; there is idiocy in the thing, in every aspect of it.

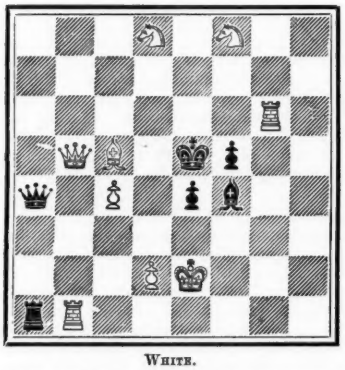
Our Chess Column.

By MEPHISTO.

PROBLEM No. 100.

By G. WOODCOCK.

BLACK.



WHITE.

White to play and self-mate in five moves.

SOLUTIONS.

PROBLEM No. 97, by C. PLANCK, p. 176.

1. Q to Kt4

If K to B6, Kt to K2 mate. K to B4, Kt to Q3 mate. K to K4, Kt to R5 mate. K to K6, Kt to Kt2 mate. P to B4, Kt takes P mate. B moves, Kt to K6 mate.

No. 98, BY J. C. S., p. 176.

1. R to Q sq.	K moves, or		B to Kt4
2. B to QKt4 (ch)	K to K4	2. B to Kt4 (ch)	Kt takes B
3. Kt to B4 mate.		3. P to B4 mate.	

ANSWERS TO CORRESPONDENTS.

*** *Please address Chess Editor.*

W. R. Edwards.—In Problem 97, if 1. Kt to Q3, K to B6, and there is no mate next move.

Correct solutions received, Problem 97, Berrow, John Watson, B. Gleam, M. T. Hooton. No. 98, John Watson. No. 99, Berrow, W., H. Seward.